## LETTERS TO THE EDITOR.

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### Locusts at Great Elevations.

THE following account of the occurrence of swarms of locusts at great elevations in the Himalaya, and these stripping birch trees, is from a privately printed record of an expedition to the north-east of Kinchinjunga, in 1891, by Mr. White, the British resident in Sikkim. That flights of locusts are carried from the plains of India up to great heights in the Himalaya is a well-known fact; but not, I think, in the numbers nor with the results to birch or other forest trees here recorded.

The Camp, April. J. D. HOOKER.

"On July 19, 1891, I crossed the Lunglala Pass, 17,400 feet . . . . On the Pass I saw the locusts that had infested Darjeeling, for the first time, though subsequently I saw them as high as 18,000 feet, where they were dying in the snow. It will be remembered that this was the year of the great plague of locusts in Malie. I heard that they had penetrated even into Tibet. On the 21st I came down as far as Tangu, 12,750 feet, where the locusts were in swarms and dying in thousands. The only plants they seemed to care about were the birches, and these they stripped bare."

### The Sandgate Landslip.

As I have just returned from Folkestone, and have had opportunities for observing the recent "landslip" at Sandgate, perhaps a note on it may be of some interest to readers of NATURE, as I do not think the explanation suggested by Mr. Blake in NATURE (vol. xlvii. p. 467) is altogether applicable to the present instance.

So far as I could see from a careful examination of the exposures, there is no trace of any movement of the solid rocks of the cliff, as these are nowhere exposed in the fissures that have been formed by the earth-movements; and my impression from all that I saw is that the "slip" has been entirely confined to the débris which has accumulated in past ages against the flank of the escarpment. On referring to the four types of Bergstürze or landslips described by Prof. A. Heim, of Zürich, some years ago in a monograph, which was summarised (with additions) by myself in the *Geological Magazine* (Decade II., vol. x. p. 160 *et seq.*), it is not difficult to identify the Sandgate incident with the first class of such phenomena, to which Prof. Heim gives the name "Schuttrutschung"; that is to say, a *slide* or *push* of an accumulation of débris (Schutt). Such accumulations often in mountain regions occur as lateral moraines or as talus; and in my paper on the origin of valley lakes (*Quar. Jour. Geol. Soc.*, vol. xxxix., February, 1883) I have attempted to show how such masses play an important part in the formation of some lakes. The Sandgate phenomenon I take to be no more than a magnified instance of what occurs in many a clayey railway-cutting, as railway-engineers know too well. There seems to be no occasion for importing the notion of "faulting" of the rocks themselves into the question. Still less rational is the notion that vibrations due to the blowing-up of one or two ships lately had anything to do with the catastrophe. The most elementary principles of mechanics explain it completely.

A mass of rock-fragments and clayey material, such as may constitute a "scree," acquires in time a certain amount of coherency from the oxidation of the irony constituents, or from the solution and redeposition of carbonate of lime (where the materials are calcareous) by carbonated atmospheric waters percolating the mass, or from both of these causes. If the mass is fairly drained internally it may retain its stable condition for any length of time, and be mistaken for a part of the solid geotectony of the district, though in cases where the materials are largely composed of decomposable silicates, it is evident that there is a tendency for the proportion of the fine slippery clay-material in the mass to increase. The consequence may be (and often is) that there is a tendency in the whole mass to settle down under the force of gravitation, and so a slow preliminary differential movement often goes on for years, before some new factor is introduced to precipitate the disaster. There can scarcely be any doubt that the new factor in this case was the excessive rainfall of last February, and the want of sufficient under-draining to carry away the water, which entered the mass of partly-compacted débris from above. A small lateral valley parallel with the general line of the escarpment had no doubt served as a catchment agent for much of this water. This supposition is borne out by the facts (1) that further to the east, where a land-drain was laid some years ago, the mass below it remained stable; (2) that above the western end of the "slip" the military hospital suffered no damage, the stability of its base being doubtless due to the complete under-draining of the site, which, as my kind friend and host Colonel Cranmer Byng informed me, was carried out before the hospital was built. It is probable, however, that at the point of maximum movement the springs from the beds which form the plateau above had much to do with the water-logging and consequent diminution of the internal friction of the débris which moved, and that the action of those springs was exceptional or excessive in the early part of this year, owing to the rise of the water-line in the ground at the back of the escarpment.

I have talked the matter over with Mr. Topley, who is an expert on all matters of Wealden stratigraphy, and he agrees generally with me as to the real nature of the phenomenon. There is one obvious and only preventive against its recurrence.

Wellington College, Berks, April 15. A. IRVING.

### "Roche's Limit."

I MUST thank your correspondent G. R. for correcting my carelessness in giving Roche's limit round the sun as about a tenth of the earth's distance, instead of about a ninetieth as it really is.

If R is the radius and D the density of a spherical planet, and $d$ the density of the tidally disturbed and infinitesimal satellite, moving in a circular orbit so as always to present the same face to the planet, then the distance at which the satellite is on the point of being broken up by the tidal forces is $2 \cdot 44 \mathrm{R} \times (\mathrm{D}/d)^{\frac{1}{3}}$.[1] This is Roche's limit, and the formula is correctly stated by G. R.

The mean density of Jupiter is about one third greater than that of water, and it does not seem unreasonable to suppose that the density of the fifth satellite may be as low as 2. This would bring the limit to 2·13 R.

Any plausible hypothesis as to the density of the stones forming Saturn's rings will no doubt bring the limit somewhat inside the outer edge of the rings.

I must plead guilty to not having made these numerical estimates whilst writing my review of Mr. See's paper. However, it still seems to me that the coincidences which I there noted are very remarkable.

The simple illustration by which G. R. obtains a fair approximation to Roche's limit is very interesting.

The satellite is replaced by two small spheres of density $d$ and radius $r$, touching one another, in line with the large sphere of density D and radius R. Suppose that when the point of contact is distant $c$ from the centre of the large sphere, the small spheres are on the point of being pulled apart; then $c$ is the approximation to Roche's limit. G. R.'s condition is that the excess of the attraction of the large sphere on the nearer small one above the attraction on the further one is equal to the attraction between the small ones. In algebraical language this becomes

$$\tfrac{4}{3}\pi \mathrm{DR}^3 . \tfrac{4}{3}\pi d r^3 \left\{ \frac{1}{(c-r)^2} - \frac{1}{(c+r)^2} \right\} = (\tfrac{4}{3}\pi d r^3)^2 . \frac{1}{4r^2}.$$

Whence

$$\frac{(c^2 - r^2)^2}{c} = 16\mathrm{R}^3 . \frac{\mathrm{D}}{d}.$$

Treating $r$ as very small we have $c = 2 \cdot 52 \mathrm{R} \times (\mathrm{D}/d)^{\frac{1}{3}}$.

If the spheres $r$ are not very small, if $\mathrm{D} = d$, and if R be taken as unit of length, the equation for $c$ becomes

$$c^4 - 2c^2 r^2 - 16c + r^4 = 0.$$

This quartic determines the approximate limit when the satellite is not infinitely small.

I shall now use this equation to find what size we must attri-

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[1] "La figure d'une masse fluide soumise à l'attraction d'un point éloigné." E. Roche. *Acad. des Sci. de Montpelier*, vol. i. (1847-50), p. 243.

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bute to the small spheres, so that all three spheres may touch one another. They touch when $c = 1 + 2r$; whence we get

$$9r^4 + 24r^3 + 22r^2 - 24r - 15 = 0,$$

the solution of which is ·85078.

Hence if the smaller spheres have their radii ·85078 of the large one, they are all three in contact, and there is no pressure between the small ones, when they revolve with proper orbital angular velocity. Now the analogue of this solution in Roche's problem is very interesting. The problem is to find the relative sizes of planet and satellite, so that where the satellite is in limiting equilibrium the two bodies shall just touch. The solution will give a fair approximation to that hour-glass figure of equilibrium of rotating fluid, which I have treated otherwise in a paper in the Philosophical Transactions (vol. clxxviii. A., p. 379). The solution would be improved, although complicated, by allowing the larger body to be also deformed.

Unfortunately the solution requires the tabulation of several functions depending on elliptic integrals. Roche made, but did not publish, tables of certain integrals, which he used for obtaining his results. It appears that the problem to which I refer did not occur to him.

Some years ago I began the computations necessary for this solution, but as it appeared to be a much more laborious task than I had anticipated, I have put the work aside until I should find leisure to attack the problem again. G. H. Darwin.

April 10.

## The Afterglows and Bishop's Ring.

I agree with your correspondents (pp. 101 and 127) that there has been a marked increase in the amount of dust in the upper regions of the atmosphere within the last few months, as evidenced by sky phenomena.

I did not notice the sunset of November 27, and it was not till the next morning I observed any increase in the dust phenomena here. About sunrise on the 28th "Bishop's Ring" was very conspicuous for the first time for a considerable period, as also were the whitish wisps in and near it, very similar to those forming such a noticeable feature of the Krakatão sunsets; but I have never again seen them so small and definite as when those sunsets first took place. The sunset of that day (November 28) was a magnificent and striking one, with a very deep pink glow. On the 30th there was a somewhat definite bright segment below the rosy glow, at first a dull buff, and then orange. This segment was a very striking feature of the earlier Krakatão sunsets, but I have rarely seen it since till that day. I noticed it again on December 4. The wisps continued to be very conspicuous up to December 13, after which date they gradually grew less so, and have now disappeared altogether.

After the middle of December I was travelling in Portugal, the Canaries, and Spain. The segment was invisible—or at any rate not a noticeable feature—after December 19 to January 30; but most of the time I was not favourably situated for seeing it on account of hills. From the last-mentioned date to February 11 (during which time I was in the neighbourhood of the Straits of Gibraltar) the sunsets—generally on a cloudless sky—were very striking, and almost nightly the orange segment was very bright and definite, though I think not quite so definite in outline as in the Krakatão sunsets, but it reminded me much of them. As I had not been in that locality before, I do not know whether such sunsets are common there, or whether the phenomena were due entirely to a general accession of dust.

Since returning to England on February 14, the segment has sometimes been visible, though much less striking than in Spain.

"Bishop's Ring" still continues very conspicuous about sunset. I have not seen it of late years when the sun has had any considerable altitude, except on the 18th ult., from 1.30 to 3.30 p.m.; I was then in Teesdale at from 1300 to 1700 feet above sea-level; it was quite plain when the sun was behind a cloud, and visible even with the sun free from clouds. It has never ceased to be visible at about sunrise and sunset since November, 1883, although at times very faint. Has it always occurred when the sun is near the horizon, and is it only because attention was called to it by its remarkable vividness at the time of the Krakatão sunsets that one has been able to see it ever since, though never before? Unlike Mr. S. E. Bishop I always see a certain amount of red in the outer margin; though in the late accession to its conspicuousness the red has been very dull, rather to be called dull brown than red. This has also been the case at times before.

One other feature of the Krakatão sunsets has occasionally been visible of late in this country, namely, the second pink glow in the western sky. This was much more striking in Teneriffe, though still much fainter than in the Krakatão sunsets.

It would appear that if this dust is the same as that seen at Honolulu, it took six weeks to get from there to Dublin and Sunderland, while the Krakatão dust took two months in reaching the south of England from Honolulu.

Sunderland, April 10. T. W. Backhouse.

## Thunderstorms and Auroral Phenomena.

I am residing in tropical Queensland, lat. 21° S., and consequently am not likely to see any auroral phenomena, particularly in the middle of our hot and rainy season; but last night between 8 and 9 p.m there occurred the following remarkable appearances, which were seen by me and several others.

There was a sharp thunderstorm with incessant lightning visible on the southern horizon, occupying a width of 10° and an altitude of from 5° to 10° above the horizon, probably from 80 to 100 miles off.

But for the distant thunderclouds the sky was clear and starlight, with a few light cirrhus clouds drifting before the north wind.

I was sitting on the lawn watching the distant flashes, when suddenly a patch or cloud of rosy light—5° to 6° in diameter—rose up from above the thunderstorm and mounted upwards, disappearing at an elevation of from 40°–45°. There were about twenty to twenty-five of these patches in the course of half an hour, sometimes three or four in quick succession; they took from one to two seconds to mount, and were not associated with any particular flash; the rosy colour contrasted strangely with the silvery light of Nubecula Major just above. There were also occasional streamers, sometimes bifurcated, of 2° in breadth, which shot up in the same way as the auroral streamers, which I have seen both in the arctic and antarctic zones.

Auroral phenomena are known to be electrical manifestations, but here were the same phenomena exhibited in connection with a thunderstorm in the tropics. Thinking this phase of electrical action worthy of note, I send you this account and enclose my card. J. Ewen Davidson.

Branscombe, Mackay, Queensland,
February 5th.

P.S.—The thunderstorm, patches of light, and streamers were distinctly *connected*; it was not a case of an ordinary aurora, with a thunderstorm interposed.

## Fossil Floras and Climate.

Sir William Dawson demonstrates that the plants of the cretaceous and tertiary series of Canada prove that the temperature of Greenland during the tertiary period was mild but not subtropical. That is sufficiently extraordinary, but geologists prefer, with strange inconsistency, the more astonishing contrast between Heer's arctic miocene palms and the glacial period. The fact is that these floras, comprising a few large-leaved evergreens and relatively tender ferns and coniferæ, are not normal in such high latitudes, but confined to localities which might have been stations on the north coast of a warm Atlantic Ocean. Therefore they perhaps require nothing more prodigious than the circulation of a gulf stream in an Atlantic isolated from the Arctic Ocean, a probable state of things at that time. At all events tertiary plants collected from near the Equator negative any generally enhanced temperature.

This applies solely to the tertiary period, when many actually living species of plants were in existence. As we recede in time species become more strange and extinct, and likely to mislead. No wise person would define, for instance, from surviving cycads the climatic conditions they may have endured when as common and widely diffused as blackberries are to-day. Even estimates based on such a group as Gleichenia may be quite inapplicable when they sustained the *rôle* now usurped by the bracken.

Sir William Dawson is aware that with even the best preserved fossil leaves, and with access to the most complete herbaria in the world, half-a-dozen different conclusions may be come to in

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